III. *On the Computation of the Effect of the Attraction of Mountain-masses, as disturbing the Apparent Astronomical Latitude of Stations in Geodetic Surveys.*

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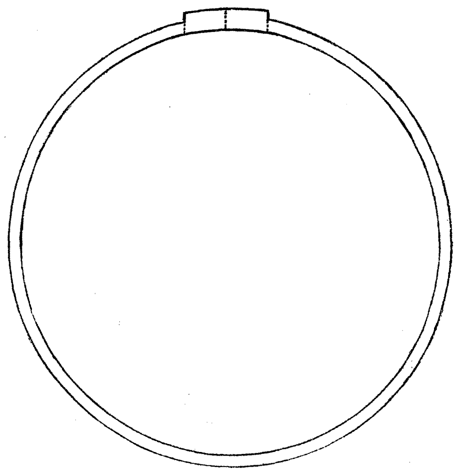
A PAPER of great ability has lately been communicated to the Royal Society by Archdeacon Pratt, in which the disturbing effects of the mass of high land north-east of the valley of the Ganges, upon the apparent astronomical latitudes of the principal stations of the Indian Arc of Meridian, are investigated. It is not my intention here to comment upon the mathematical methods used by the author of that paper, or upon the physical measures on which the numerical calculation of his formulæ is based, but only to call attention to the principal result; namely, that the attraction of the mountain-ground, thus computed on the theory of gravitation, is considerably greater than is necessary to explain the anomalies observed. This singular conclusion, I confess, at first surprised me very much.

Yet, upon considering the theory of the earth's figure as affected by disturbing causes, with the aid of the best physical hypothesis (imperfect as it must be) which I am able to apply to it, it appears to me, not only that there is nothing surprising in Archdeacon Pratt's conclusion, but that it ought to have been anticipated; and that, instead of expecting a positive effect of attraction of a large mountain mass upon a station at a considerable distance from it, we ought to be prepared to expect no effect whatever, or in some cases even a small negative effect. The reasoning upon which this opinion is founded, inasmuch as it must have some application to almost every investigation of geodesy, may perhaps merit the attention of the Royal Society.

Although the surface of the earth consists everywhere of a hard crust, with only enough of water lying upon it to give us everywhere a *couche de niveau*, and to enable us to estimate the heights of the mountains in some places, and the depths of the basins in others; yet the smallness of those elevations and depths, the correctness with which the hard part of the earth has assumed the spheroidal form, and the absence of any particular preponderance either of land or of water at the equator as compared with the poles, have induced most physicists to suppose, either that the interior of the earth is now fluid, or that it was fluid when the mountains took their present forms. This fluidity may be very imperfect; it may be mere viscidity; it may even be little more than that degree of yielding which (as is well known to miners) shows itself by changes in the floors of subterraneous chambers at a great depth when their width exceeds 20 or 30 feet; and this yielding may be sufficient for

my present explanation. However, in order to present my ideas in the clearest form, I will suppose the interior to be perfectly fluid.

Fig. 1.



In the accompanying diagram, fig. 1, suppose the outer circle, as far as it is complete, to represent the spheroidal surface of the earth, conceived to be free from basins or mountains except in one place; and suppose the prominence in the upper part to represent a table-land, 100 miles broad in its smaller horizontal dimension, and two miles high. And suppose the inner circle to represent the concentric spheroidal inner surface of the earth's crust, that inner spheroid being filled with a fluid of greater density than the crust, which, to avoid circumlocution, I will call *lava*. To fix our ideas, suppose the thickness of the crust to be ten miles through the greater part of the circumference, and therefore twelve miles at the place of the table-land.

Now I say, that this state of things is impossible; the weight of the table-land would break the crust through its whole depth from the top of the table-land to the surface of the lava, and either the whole or only the middle part would sink into the lava.

In order to prove this, conceive the rocks to be separated by vertical fissures at the places represented by the dotted lines; conceive the fissures to be opened as they would be by a sinking of the middle of the mass, the two halves turning upon their lower points of connexion with the rest of the crust, as on hinges; and investigate the measure of the force of cohesion at the fissures, which is necessary to prevent the middle from sinking. Let C be the measure of cohesion; C being the height, in miles, of a column of rock which the cohesion would support. The weight which tends to force either half of the table-land downwards, is the weight of that part of it which is above the general level, or is represented by 50×2. Its momentum is $50 \times 2 \times 25 = 2500$. The momenta of the "couples," produced at the two extremities of one half, by the cohesions of the opening surfaces and the corresponding thrusts of the angular points which remain in contact, are respectively $C \times 12 \times 6$ and $C \times 10 \times 5$; their sum is $C \times 122$. Equating this with the former, $C = 20$ nearly; that is, the cohesion must be such as would support a hanging column of rock twenty miles long. I need not say that there is no such thing in nature.

If, instead of supposing the crust ten miles thick, we had supposed it 100 miles thick, the necessary value for cohesion would have been reduced to $\frac{1}{5}$th of a mile nearly. This small value would have been as fatal to the supposition as the other. Every rock has mechanical clefts through it, or has mineralogical veins less closely connected with it than its particles are among themselves; and these render the cohesion of the firmest rock, when considered in reference to large masses, absolutely

insignificant. The miners in Cornwall know well the danger of a "fall" of the firmest granite or killas* where it is undercut by working a lode at an inclination of 30° or 40° to the vertical.

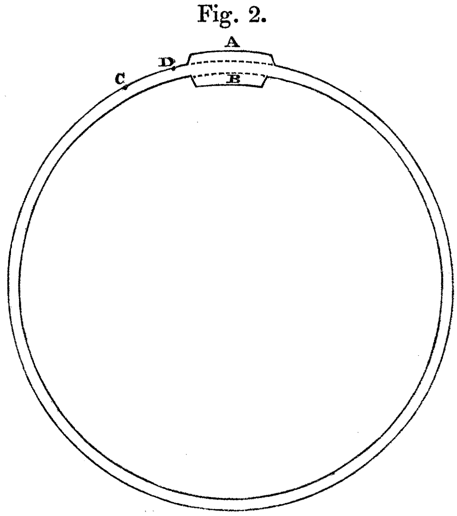
We must therefore give up the supposition that the state of things below a table-land of any great magnitude can be represented by such a diagram as fig. 1. And we may now inquire what the state of things really must be.

The impossibility of the existence of the state represented in fig. 1 has arisen from the want of a sufficient support of the table-land from below. Yet the table-land does exist in its elevation, and therefore it *is* supported from below. What can the nature of its support be?

I conceive that there can be no other support than that arising from the downward projection of a portion of the earth's light crust into the dense lava; the horizontal extent of that projection corresponding rudely with the horizontal extent of the table-land, and the depth of its projection downwards being such that the increased power of floatation thus gained is roughly equal to the increase of weight above from the prominence of the table-land. It appears to me that the state of the earth's crust lying upon the lava may be compared with perfect correctness to the state of a raft of timber floating upon water; in which, if we remark one log whose upper surface floats much higher than the upper surfaces of the others, we are certain that its lower surface lies deeper in the water than the lower surfaces of the others.

This state of things then will be represented by fig. 2. Adopting this as the true representation of the arrangement of masses beneath a table-land, let us consider what will be its effect in disturbing the direction of gravity at different points in its proximity. It will be remarked that the disturbance depends on two actions; the positive attraction produced by the elevated table-land; and the diminution of attraction, or negative attraction, produced by the substitution of a certain volume of light crust (in the lower projection) for heavy lava.

Fig. 2.



The diminution of attractive matter below, produced by the substitution of light crust for heavy lava, will be sensibly equal to the increase of attractive matter above. The difference of the negative attraction of one and the positive attraction of the other, as estimated in the direction of a line perpendicular to that joining the centres of attraction of the two masses (or as estimated in a horizontal line), will be proportional to the difference of the inverse cubes of the distances of the attracted point from the two masses.

* A "fall" occurred in the Dolcoath mine, while I was engaged there with Messrs. WHEWELL, SHEEPSHANKS, and other friends, on pendulum-experiments, in 1828.

Suppose then that the point C is at a great distance, where nevertheless the positive attraction of the mass A, considered alone, would have produced a very sensible effect on the apparent astronomical latitude, as ten seconds. The effect of the negative attraction of B will be $10'' \times \frac{CA^3}{CB^3}$; and the whole effect will be $10'' \times \frac{CB^3 - CA^3}{CB^3}$, which probably will be quite insensible.

But suppose that the point D is at a much smaller distance, where the positive attraction of the mass A would have produced the effect n''. The whole effect, by the same formula, will be $n'' \times \frac{DB^3 - DA^3}{DB^3}$, or $n'' \times \left(1 - \frac{DA^3}{DB^3}\right)$; and as in this case the fraction $\frac{DA}{DB}$ is not very nearly equal to 1, there may be a considerable residual disturbing attraction. But even here, and however near to the mountains the station D may be, the real disturbing attraction will be less than that found by computing the attraction of the table-land alone.

The general conclusion then is this. In all cases, the real disturbance will be less than that found by computing the effect of the mountains, on the law of gravitation. Near to the elevated country, the part which is to be subtracted from the computed effect is a small proportion of the whole. At a distance from the elevated country, the part which is to be subtracted is so nearly equal to the whole, that the remainder may be neglected as insignificant, even in cases where the attraction of the elevated country itself would be considerable. But in our ignorance of the depth at which the downward immersion of the projecting crust into the lava takes place, we cannot give greater precision to the statement.

In all the latter inferences, it is supposed that the crust is floating in a state of equilibrium. But in our entire ignorance of the *modus operandi* of the forces which have raised submarine strata to the tops of high mountains, we cannot insist on this as absolutely true. We know (from the reasoning above) that it will be so to the limits of *breakage* of the table-lands; but within those limits there may be some range of the conditions either way. It is quite as possible that the immersion of the lower projection in the lava may be too great, as that the elevation may be too great; and in the former of these cases, the attraction on the distant stations would be negative.

Again reverting to the condition of *breakage* of the table-lands, as dominating through the whole of this reasoning, it will be seen that it does not apply in regard to such computations as that of the attraction of Schehallien and the like. It applies only to the computation of the attractions of high tracts of very great horizontal extent, such as those to the north of India.

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